Linear Collider Workshop 2000 Summary

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Abstract. We summarize some of the main physics questions that will serve to define the linear e^+e^- collider program, and comment on issues that confront the world community in making such a collider a reality.

The 1999 International Workshop on Linear Colliders (LCWS) at Sitges, Barcelona [1,2] demonstrated the wide range of physics opportunities at a 500 – 1000 GeV linear e^+e^-collider (LC). Since that time, there has been considerable progress on R&D and technical proposals for a collider, and in further exploring the physics potential. The 2000 LCWS [3] at Fermilab gave the opportunity to update the physics case and detector needs, and to review accelerator developments.

The linear collider proposals under discussion are TESLA [4], JLC [5] and NLC [6]. TESLA proposes superconducting cavities operating at 1.3 GHz and effective gradients of 22 MV/m. The JLC or NLC X-band proposals employ warm accelerating structures at 11.4 GHz with 50 MV/m. The JLC C-band machine would operate at 5.7 GHz and 34 MV/m. Each envisions a first phase of operation at up to $\sqrt{s} = 500$ GeV. The luminosity (at 500 GeV) expected for TESLA is $\mathcal{L}=$ 3.4 $\times 10^{34}$cm^{-2}s^{-1}; for the X-band JLC or NLC, $\mathcal{L}=$ 2.2 $\times 10^{34}$cm^{-2}s^{-1}; and for the C-band JLC, $\mathcal{L}=$ 4.3 $\times 10^{33}$cm^{-2}s^{-1}. TESLA could be upgraded to about 800 GeV; the X-band JLC/NLC is upgradeable to about 1000 GeV with known technology. Recall that $\mathcal{L}=$ 1 $\times 10^{34}$ cm^{-2}s^{-1}gives 100 fb^{-1}accumulated data for a 'standard operating year' of 10^7 s. Future higher energy e^+e^-colliders may depend upon the use of high intensity, low energy drive beams as a source of RF power; this concept has been pioneered by the CLIC [7] R&D program at CERN.

The superconducting TESLA machine and warm cavity JLC/NLC designs differ in several important respects that could influence a final choice of collider technology. However, the physics case likely depends only weakly on this choice, once the maximum energy and luminosity are fixed. Thus, the work across the world to understand the physics capabilities and delineate the parameters of detectors are relatively interchangeable. This commonality of the worldwide study of the linear collider program has been a strong unifying force for the community.

1. The Linear Collider physics program

The many contributions and plenary summaries at this workshop make a strong and detailed case for the LC physics capability, and I do not repeat this in depth here. The range of physics issues for LC study were summarized as part of the charge to this workshop [8,9]. There has also been a recent discussion of a first phase of the LC at 500 GeV, outlining the case that substantial new physics will be accessible there [10].

1.1 Higgs studies

The clearest and most pressing physics issue at the LC is the full study of the Higgs sector. In both the Standard Model (SM) and its supersymmetric extensions, we confidently expect a Higgs boson with mass below 200 GeV. In most strong coupling models, or models with large extra dimensions, Higgs-like objects are expected at a few hundred GeV. For all but some special corners of parameter space, such states should be discovered, and the mass determined to good precision at the LHC. However the LHC will likely fail to fully delineate the Higgs: branching ratios to fermions, J^{PC} determinations, CP-violation properties, the total width, and Higgs self-couplings are all difficult to determine, or are inaccessible, at the LHC. In the case that the Higgs is embedded in supersymmetry, further measurements of the other Higgs masses, and the ratio of up- and down-quark couplings ($\tan\beta$) will also be difficult in a model-independent context. Indeed, as we move away from the simplified supersymmetric paradigms to consideration of the full supersymmetric model with over 100 soft parameters, we will need many measurements in the Higgs (and sparticle) sector to learn what these parameters are in Nature.

The LC should be capable of making a full portrait of the Higgs bosons, given enough energy to produce them [11,12]. The LC will make only marginal improvements in the Higgs mass determination over the LHC [14]. However, it will allow measurement of the total width to a few percent, even for low mass Higgs where the LHC has no capability due to finite energy resolution. The total width is a key parameter for sensing potential new decay channels, multiple Higgs states, or non-SM effects. Though the LHC can rule out spin 1 through observation of $H \to \gamma\gamma$, and can provide some quantum number information for high mass Higgs through the ZZ final state angular correlations, the LC can give full quantum number determination for any Higgs mass using polarized beams and threshold scans.

Determination of the branching ratios of the Higgs to fermion pairs will be crucial

for distinguishing the SM from supersymmetric or other SM extensions. Studies [13,11] show that for M_{Higgs} below 150 GeV, the branching ratios for SM Higgs decays into $b\bar{b}$, $c\bar{c}$, gg, $\tau^+\tau^-$, WW can be determined to 5 – 10% with 500 fb^{-1}. The $\gamma\gamma$ branching ratio can be measured for low Higgs mass (< 120 GeV) to perhaps 20%. The Higgstrahlung and WW fusion processes can be differentiated to give information on the g_{HWW} and g_{HZZ} couplings. Operating the LC above the $t\bar{t}H$ threshold will give the top Yukawa coupling to the 10% level. Finally, the crucial self-couplings of the Higgs bosons, key to understanding the character of the Higgs potential, can be determined through the study of double Higgs bremsstrahlung; these measurements will be statistics limited [11] and may require very good jet energy resolutions. The full set of possible Higgs measurements would benefit from further study using complete realistic simulations of potential detectors.

The combination of all this high precision Higgs information can be used in an analogous way to that of the LEP/SLC/Tevatron data constraining the SM. Departures from SM orthodoxy for branching ratios, total width, or couplings would be expected in any new physics model, so the Higgs sector measurements will have high priority. For example, the branching fraction data at a 500 GeV LC is capable of sensing a supersymmetric pseudoscalar Higgs (A) up to about 700 GeV [13]. Understanding the properties of a Higgs candidate discovered at the Tevatron or LHC and seeking departures from the SM will form the backbone of the LC program for several years, and gives a strong justification for acquiring high luminosity samples. This program will occupy the initial years of LC operation if a Higgs candidate emerges below about 200 GeV. The Higgs sector measurements also will require substantial luminosity accumulations at energies at ~ 1 TeV to determine the $t\bar{t}H$ Yukawa coupling, and to explore the heavier Higgs states if supersymmetry is part of the new physics.

1.2 Studies of the supersymmetric partners

If supersymmetry is responsible for electroweak symmetry breaking, we confidently expect observable effects at the LHC [14]. The LHC should be able to determine mass differences for particles in the cascade decays of the strongly produced squarks and gluinos, and thus to measure Susy model parameters given an assumption of the model class, sometimes to percent level precision. The LHC will however not typically be sensitive to the heavy gaugino or Higgs states, will not see sleptons unless they are prominant in squark or gluino decays, will typically not study the sneutrinos, and will likely not be capable of measuring sparticle quantum numbers, gaugino or sfermion mixing angles, or probe the CP-violating phases.

The LC measurements of Susy particle properties and mixings, leading to determination of the underlying Susy model parameters will extend the LHC studies dramatically [15,16]. The ability in the LC to polarize the electron (and perhaps positron) beam allows selection of particular subprocesses of interest, or permits suppression of SM backgrounds. The ability to set the parton cm energy above successive sparticle pair thresholds will help disentangle backgrounds from Susy itself. Precise measurements at the LC will in several cases improve the utility of the LHC

results; for example, knowledge of the LSP mass from the LC will allow LHC mass differences to be converted into masses. The LC and LHC are often complementary – the LHC should access the sparticle states that couple to color, up to very high mass, whereas the LC will excel in studies of the slepton and gaugino sectors.

If the LHC shows that supersymmetry has a key role in EWSB, the next question of paramount importance is how the supersymmetry breaking occurs, and at what scale the symmetry is broken. Though most studies of Susy are now conducted in a specific simplifying framework such as gravity mediating Susy, gauge mediation, anomaly mediation or gaugino mediation with relatively few parameters, the real model is likely to be considerably more complex. To some extent, the sparticle mass patterns can indicate the general type of model chosen by Nature [15], but to fully realize the model more experimental information will be needed. We recall that the minimal supersymmetric model contains over 100 arbitrary parameters, and that a full understanding of Susy requires that we determine them all. Typically, the CP-violating phases are set to zero in simulation exercises, but it may well be that non-zero phases are required to explain the CP violation patterns seen in the K and B systems, and to explain the baryon-antibaryon asymmetry in the universe [17]. Measurements of CP-violation in supersymmetry may be the next new frontier in flavor physics!

Measuring a large set of Susy observables – masses, mixing angles in chargino, neutralino, stop and stau sectors, branching fractions, Higgs sector parameters etc. – will be necessary to fully determine the Susy model. It is likely that the measurement of the Susy model parameters will form a productive interface to string theoretic models, giving experimental guidance to Planck scale physics theory, and helping point the way to identifying the intermediate scales of new physics between the TeV and Planck scales [17]. Obtaining information on the multitude of new Susy parameters will require extensive and precise measurements of as many new Susy particles as we can produce. Thus, the energy scale at which we may be confident that the sparticles can be produced in the LC is a critical question. There are at present only plausibility arguments to guide us. If supersymmetry is to produce EWSB and yield the observed Z mass without 'excessive' fine tuning, then it follows that we are likely to produce the lighter chargino and neutralinos, and the sleptons, at a 500 GeV linear collider [10]. If the LSP is to provide the candidate for dark matter in the universe, light sparticles are preferred (though some of the parameter space yielding Susy dark matter requires raising the LC energy to 1 TeV or a little higher [18]). If Susy is to provide the CP violation needed for cosmic baryon asymmetries, light sparticles are favored.

However, in a model as rich and complex as supersymmetry, there are few guarantees on where we will find the spectrum of new particles. More to the point, it is almost certain that some of them will be inaccessible at a 500 GeV collider, and that to understand the Susy symmetry breaking, higher energy will be required. Although finding supersymmetry will bring powerful justification for a first stage of the LC at about 500 GeV, it seems likely that it will also reinforce the case for energy upgrade to at least 1 TeV in a subsequent phase.

1.3 Non-supersymmetric extensions to the Standard Model

Popular though supersymmetry is as a framework for going beyond the Standard Model, there is at present no hard experimental evidence to support this solution to electroweak symmetry breaking. Indeed, in the past several years alternative models for EWSB [19,20] have been elaborated in a way that conforms to the precision EW data from current experiments, and which possess strong intuitive appeal for many. Thus it is important to gauge the strength of the LC in exploring the consequences for such models. Two main non-Susy themes have been developed theoretically: models in which there is a new strong coupling sector, typically at the several TeV scale, and models in which the some of the string-inspired extra spatial dimensions can be larger than the Planck length and enter into the phenomenology of EWSB.

The strong coupling models emerged around 1980, based upon analogies with QCD or superconductivity. The effects of the SM Higgs boson could be mimicked by new physics at the TeV scale, often producing composite Higgs-like particles. The precision electroweak data accumulated over the past 20 years place stringent constraints on the properties of such theories, but viable models remain [21]. The masses of the composite Higgs states depend on the model envisioned, but are typically in the range below 500 – 600 GeV. Its couplings to ordinary particles resemble those of the SM, but new couplings arise that give observable effects. In particular, anomalous $WW\gamma$ and WWZ couplings are expected in the range $\Delta\kappa$, λ = few $\times 10^{-3}$ that should be observable through WW pair production in e^+e^- collisions, but would be problematic for LHC. Modification of the SM $Z\bar{t}t$ couplings would also be expected at the 5 – 10% level, and these should be accessible at the LC using polarized electron beams. Strong coupling theories typically modify WW scattering for a LC operating above 1 TeV, and at the LHC.

Models incorporating large extra dimensions suggest many potential new signatures, depending on which particles propagate into the bulk and the size and number of extra dimensions [22]. In this space of model types, many observable effects could come into play, including direct observation of Kaluza Klein (KK) towers of gravitons that simulate contact interactions, observable KK towers of gauge bosons resembling excited Z bosons, modifications to the angular and mass distributions of fermion or boson pairs, or modification of scattering amplitudes at large momentum transfers. Typically the LHC can probe new particles to somewhat higher mass, but the LC can disentangle the character of the states more efficiently, and can probe new physics in the lepton sector that is not available in hadron machines.

The unifying thread for the multitude of strong coupling and string inspired models is the potential need for higher energy to directly access the new physics. However, we have learned from the past that the constraints placed by precision measurements at the 100 GeV scale have very substantially limited the space of potential new models of these types. Thus, if future experiments suggest that it is in these directions that we must go, the precision measurement arena will surely

be important. Samples of 10^9 or more Zs should allow crucial improvements of many of the electroweak parameters [23]. Operation of the LC at the $t\bar{t}$ and WW thresholds will give great improvements in our knowledge of the top quark and W boson mass, and these are also crucial for constraining new models. Moreover, if there is a Higgs-like state to study, it too should be regarded as fertile ground for precision measurements of its properties, as these too give powerful guidance on the character of the new physics. Thus, even in the case that we are in a realm of exploring non-SM physics whose new particle spectrum lies above the initial energy of the collider, there are many critical measurements at lower energies needed for delimiting this new world and for pointing to the productive avenues for its exploration.

1.4 Other physics opportunities

For most, elucidating the EWSB mechanism, and pointing the way to physics at higher mass scales is the key element of the LC program. There are however many other physics topics that will form important parts of the LC program. Some of these could be raised in importance if the particles associated with EWSB are not clearly visible at the LHC or in the first stage of the LC program. As mentioned above, high statistics samples of Z's may be crucial to constrain new physics. Improved precision of the W boson and top quark masses can be obtained from runs at the appropriate pair thresholds, and these too may be a key ingredient in unravelling the puzzle if Susy particles are not seen. A large sample of Z bosons ($\mathcal{O}(10^{10})$) would give impressive samples of clean $b\bar{b}$ events with which to pursue studies of the CKM matrix parameters, CP -violation, and rare b decays.

Electroweak measurements of W/Z and $t\bar{t}$ final states can probe for new short distance effects. The anomalous coupling studies of W and top will be more sensitive than those available from the LHC. Large samples of $t\bar{t}$ can be used to reconstruct the spin structure of the interaction and probe for CP-violating effects [24].

As has been true at lower energy e^+e^- colliders, the LC will enable many precise tests of QCD and hadronic structure [25]. The precision to which the strong coupling constant can be determined is estimated at 1%, using a variety of established methods. The LC will open up new opportunities for the study of $\gamma\gamma$ scattering and photon structure. The study of low-x resummed BFKL effects should be particularly clean, and will offer a new opportunity to understand QCD dynamics in non-perturbative regimes. Gluon radiation in top quark production and decay offers new windows for probing non-perturbative effects in QCD. Studies of $t\bar{t}$ and single top quark production in $\gamma\gamma$ collisions should complement those in e^+e^- collisions.

Perhaps the most likely areas of physics beyond the usual EWSB topics that will occupy us at the LC are those that are as yet dimly, if at all, in view. It has been refreshing in the past few years to see the wealth of new speculative models suggesting observable phenomena at the LC. It is clear that the phenomenology associated with large extra spatial dimensions will grow richer in the near future. At this meeting, we have heard exotic suggestions [19] regarding new contributions to the general relativistic metric ('torsion'); many variants of the sets of particles

propagating in extra dimensions; suggestions of non-commutative field theories with observable consequences; modifications to the equivalence principle; and search for 'maximal weirdness' (quite likely inaptly named, since theorists may be counted on to outdo themselves in weirdness every year or two). The point of course is not that any of these conjectures has a high probability to be realized in Nature, but that there will be a large variety of new ideas, and that the LC will have unique capabilities to test them.

1.5 Physics summary

The physics opportunities at the LC are rich and varied. Although we cannot state with absolute certainty that the source of EWSB will be accessible at the 500 GeV LC, it is an extraordinarily good bet. The SM Higgs can be studied in depth. Supersymmetry is open for study, both in the Higgs sector and at least the lighter gauginos and sfermions. Observable effects from new strong interactions, or from models with large extra dimensions, are found in virtually all existing models. In any case, the observations at 500 GeV should clearly point the way for upgrades in collider energy.

We should not expect that the LC will necessarily discover the new phenomena related to EWSB. The Higgs or supersymmetry will likely be first seen at hadron colliders. But we should not take discovery of the new physics as the main criterion for deciding to build a linear collider. One may argue that LEP/SLC provided no new 'discoveries' (the demonstration of three neutrino species with mass below $m_Z/2$ and normal $SU(2) \times U(1)$ quantum numbers is an arguable exception). Yet there can be no argument that these experiments have dramatically and fundamentally altered our understanding of particle physics. Similarly, we are confident that the LC will provide us with a true understanding of what the Higgs boson is, will delineate the nature of the supersymmetric world and point to its symmetry breaking characteristics, or will expose the nature of strong coupling – even if these phenomena are first observed at the LHC. For each of the broad cases studied so far, the LC is essential to understand the new physics beyond the SM.

2. Experimental issues

There are many issues related to the collider operating conditions or the detectors that require fuller discussion in the worldwide community. Some of these are affected by the specific physics scenario that we find ourselves in, so flexibility should be retained so that later incorporation of options can be made.

2.1 Positron polarization

The need for electron polarization has long been recognized; $|\mathcal{P}_{e^-}| = 0.9$ could be achievable using improved strained GaAs targets. Positron polarization might be achieved using pair production from polarized photons from undulator magnets, or from backscattering off high power lasers; $|\mathcal{P}_{e^+}| = 0.5$ seems a sensible goal.

Positron polarization would give advantages similar to those from electron polar-

ization – the ability to suppress backgrounds and enhance specific signal subprocesses. Examples from Susy include [15]:

- The reactions $e^+e^- \to \widetilde{\chi}_i^+ \widetilde{\chi}_i^-$, $\widetilde{\ell}\widetilde{\ell}$ can be dialled from mostly $\widetilde{\ell}_R\widetilde{\ell}_{L/R}$ to dominantly $\widetilde{\ell}_L\widetilde{\ell}_L$ and $\widetilde{\chi}_i^+ \widetilde{\chi}_i^-$ as $\mathcal{P}_{e^+}$ is varied from -0.6 to $+0.6$.

- The precision with which $\widetilde{t}$ mixing can be measured is enhanced noticeably using positron polarization.

- The polarization dependence of $\widetilde{\chi}_1^0\widetilde{\chi}_2^0$ differs between minimal Susy and extended models.

- Allowing positron polarization gives improved ability to measure mixings in the gaugino sector, and thus is a key contributor to analyses that determine the nature of Susy breaking mechanisms.

The Higgs pair production cross section, used to measure the Higgs potential parameters, is enhanced by up to a factor of two with positron polarization [11,12]. If much better precision on $\sin \theta_W^2$ is required, new measurements of A_{LR} at the Z-pole will be needed; the errors arising from uncertainty on beam polarizations are greatly reduced if the positron beam can also be polarized [23,20].

The need for positron polarization as a tool to increase precision and to disentangle rival processes is sufficiently widespread that it is highly desirable that such a capability be allowed by the design of the accelerator, even if it is not implemented at the very beginning.

2.2 $\gamma\gamma$ collisions

$\gamma\gamma$ (or $e\gamma$) collisions can be made by backscattering high power laser beams from the electron beams [26,27]; polarized photon beams at about 80% of the primary electron beam energy can be produced. The $\gamma\gamma$ luminosity is about 0.4 times that for e^+e^-, and is peaked within about 15% of the maximum energy. Recent developments in lasers are promising, but considerable work remains to develop the appropriate mirrors, beam masks etc. Special care must be given in detector design to accomodate the high flux of e^+e^-halo backgrounds;

The need for $\gamma\gamma$ collisions, like that for $\mathcal{P}_{e^+}$, ranges widely over the potential LC program. The total width of the Higgs boson is a key measureable, and if the Higgs mass is below 200 GeV, is inaccessible for the LHC. Measurement of $\sigma(\gamma\gamma \to H)$ and $BR(H \to \gamma\gamma)$ should give the total width error of 5% (in 200 fb^{-1}). Higgs production from circularly polarized $\gamma\gamma$ collisions allows tests of CP violation in the Higgs sector. $\gamma\gamma \to$ Susy H/A will help distinguish the scalar and pseudoscalar states. Chargino production in $\gamma\gamma$ collisions offers clean determination of gaugino mass matrix parameters. In many cases, new particle production is enhanced in $\gamma\gamma$ collisions relative to e^+e^-.

Once again, we should foresee that some of the physics that evolves at the LC could be enhanced strongly by the use of $\gamma\gamma$ collisions. R&D on this option

should be vigorously pursued and the option to add such collisions after the initial e^+e^- operation should be retained.

2.3 Low energy collisions

We have noted that in some physics scenarios we would benefit from substantial new samples of Z bosons. These could of course be obtained at loss of peak luminosity by reduction of the beam energies. Recent implementations of the NLC have investigated an IR layout in which the full energy beams are brought with little or no bend to a collision point (thus facilitating later upgrades of the energy of the collider); a lower energy beam can be envisioned that is picked off early in the linacs and accelerated 'for free' using unused portions of the power cycle. Because of the permanent magnets foreseen for focussing the linac beams, only about a factor of two variability of beam energy would be possible on the short term. Thus, there is the potential for two ranges of collision energy: from $0.5 - 1.0$ times full energy, and from the Z-pole to half the full energy (up to perhaps a maximum of 500 GeV).

A Giga-Z sample, obtainable with 30 fb^{-1} of data, should make substantial improvements in the precision of many EW parameters [23]. Factors of 15 improvement are foreseen for $\sin\theta_W^2$ and A_b. Factors of $2 - 5$ improvement should be possible for $\Gamma(Z \to \ell\ell)$ and R_b. 100 fb^{-1} of data at the WW threshold could improve the W mass error to below 10 MeV. Taken together, these should improve the knowledge of radiative corrections (the S and T parameters) by about a factor of 8, to the point where extremely tight constraints can be placed on potential models for new physics. It is likely that ultimately one would want to obtain the Giga-Z sample; in the case that we do not have manifest new physics at the LHC and LC (*e.g.* supersymmetry) it will be imperative to obtain these data. It remains an open question for discussion whether an optimum program has distinct experiments focussed on the upper and lower ranges of energies.

2.4 e^-e^- collisions

It should be straightforward to provide e^-e^- collisions at the LC, though the luminosity will be somewhat reduced relative to e^+e^- owing to the absence of the self focussing effects. Some supersymmetry studies, searches for new phenomena such as lepton compositeness, flavor mixing effects ($\tilde{e} \to \mu\tilde{\chi}^0$, strong WW scattering, or searches for large extra dimensions are enabled or enhanced by e^-e^- collisions. The nature of new physics found in the initial e^+e^- running will indicate how useful the e^-e^- operation might be, but clearly one would not want to preclude such running in the design phase of the machine.

2.5 Free electron laser physics

Beams of very short wavelength, extremely high peak brightness, photons are desirable for a broad range of physics, chemistry, biology and material science applications. Beams based upon free electron lasers would dramatically increase the scope of synchrotron light applications beyond the present third generation light sources. The full range of applications are only now beginning to be understood

[28]. They include the study of 'warm plasmas' such as found in planetary interiors or ion beams; very high-field atomic physics exploring exotic atomic states and non-linear effects; nanoscale dynamics in condensed matter involving short time correlations and collective effects; femtosecond probes of chemical reactions; and perhaps most exciting, the possibility of structural studies of biomolecules such as protein complexes with angstrom level resolution.

In the US, the LCLS project is proposed to build a self-amplified spontaneous emission (SASE) free electron laser based on the last third of the SLAC linac. The TESLA project has incorporated an ambitious FEL component using electron beams of up to 30 GeV, transported to special experimental halls to the side of the high energy physics IRs. The LCLS could come into first operation in 2006, and will begin to map out the range of experiments that are feasible in the new very high brightness regime. A linear collider project starting sometime later could build on the experience of this pilot project, and provide facilities for a broad international community to explore a wide range of studies. We should nurture the connection between the LC project and this broader scientific community to promote new opportunities for structural studies of matter. The joint interests of the two communities should interfere positively to give a more compelling argument for the LC project.

2.6 Detector issues

Work has continued in the past two years to define the scope of linear collider detectors, to conduct R&D on specific detector technologies, and to simulate performances for proposed subdetectors. Recent developments for detector simulations [29], vertex detectors [30], tracking detectors [31], calorimetry [32], muon detectors [33], data acquisition [34], and machine interface [35] are summarized elsewhere in the proceedings.

We assume that there will be two detectors operating in the LC; for most, the need for independent confirmation of new discoveries and important measurements dictates this. The broad outlines of the LC detectors have been understood for some time, based on the performance of existing collider detectors and the general goals of the LC physics program. Each linear collider proposal will be accompanied by general detector designs, sufficient to estimate their scope and cost. However, it is wise to defer specific design choices and optimizations until such time as real detector collaborations are established, and the full range of interconnected optimizations can be made by those responsible for building the detectors.

In the meantime, there are some interesting design choices that emerge, and these deserve more attention for simulation and understanding the physics needs. The choices of vertex detector technology at present include CCDs and variants of active silicon pixel devices. It is clear that the best affordable vertex detector will be needed for the LC physics program, with the main constraints coming from b, c tagging (*e.g.* Higgs branching ratio measurements, $t\bar{t}H$ coupling, Susy AH^0 production, etc.). Since the need for the best performance vertex detector is clear, emphasis is needed on the R&D program to develop commercially viable detectors,

packaging and readout structures.

The other major detector issue concerns calorimetry. For physics studies such as Higgs pair production in association with a Z boson (to measure the Higgs self couplings) there is a premium on the best possible jet energy resolution. For a light Higgs, the decay is dominantly $H \to b\bar{b}$. The $Z \to q\bar{q}$ decay is preferrable due the higher branching ratio. Disentangling the jet combinatorics and suppressing backgrounds depends directly upon the jet energy (and dijet mass) resolution. A study [36] showed that as the jet energy resolution improved from $60\%/\sqrt{E}$ to $30\%/\sqrt{E}$, the background level decreased by a factor of 6 and the precision on the di-Higgs cross section improved by a factor of 1.6. Studies of an 'energy-flow' calorimeter have been conducted in which a very finely segmented calorimeter with small Molière radius (small transverse shower spread) is used to identify clusters caused by charged tracks, and replace their calorimeter energy deposit with the corresponding charged track measurement. Other advantages of such a finely segmented calorimeter may include particle identification algorithms for τs and photons. The cost of the finely divided calorimeter is however large. The TESLA detector envisions active layers of silicon pads (about 1 cm^{-2}) with tungsten absorber plates.

It is essential to further refine the studies of the energy-flow calorimeter with full simulations and tests to verify the performance characteristics, and to explore the tradeoffs further. An added consideration is the extent to which the radius of such a calorimeter may be reduced to help control costs; this will in turn place added burdens on the interior tracking region detectors.

3. Scenarios for new physics at the linear collider

At present we do not have a clear understanding of the Higgs sector and electroweak symmetry breaking, so the detailed plan for experimentation at the LC and the need for upgrades to the accelerator are not wholly understood. We can however describe several possible scenarios for the way that physics could play out. It is important to develop representative examples, both to frame the LC proposals and to assure ourselves that in all imaginable scenarios the LC has a crucial role to play. The charge to this workshop [9] and the recent discussion of the 500 GeV program [10] began the examination of such scenarios. We briefly review some of them here, realizing that fleshing these out is an important role for the planning exercises now underway in each region.

1. *There is a Higgs below 130 GeV and evidence for supersymmetry has been found at the Tevatron/LHC.*

   The highest initial priority will be to measure the Higgs properties thoroughly at 500 GeV or below. The character of the Susy states accessible at 500 GeV should be determined.

   The energy upgrade to about 1 TeV will surely be needed. Measuring the Higgs Yukawa couplings to the top quark requires 700 – 800 GeV. The remaining sparticles and heavy Higgs must be observed and studied. The full exploration

of the Susy breaking sector will likely require the study of the more massive gauginos and sfermions.

2. *There is a Higgs boson below 180 GeV, but no evidence for supersymmetry.*

   Again, the Higgs boson parameters must be fully measured at 500 GeV; high precision is desired since these parameters may hold crucial clues on the nature of physics beyond the SM.

   Substantial operation at the Z pole, and WW, $t\bar{t}$ thresholds will be desired since the precision constraints on non-SM models can substantially limit possible new theories. The WWV and ttZ anomalous couplings should be measured with high precision as these are also indicators of possible new physics.

   Later operation at the highest available energy is likely needed, to study the anomalous ttH couplings, to seek deviations of WW scattering from SM EW production, to look for evidence of large extra dimensions, etc.

3. *There is a Higgs boson between 180 – 300 GeV, and no evidence for supersymmetry.*

   We note that in this variant of scenario 2, we have departed from the SM since the Higgs mass now exceeds the precision measurements limit.

   At 500 GeV, we will want to measure as much as we can about the Higgs, but the fermionic branching ratios are likely inaccessible. (This would be disappointing since one of the hallmarks of the Higgs is its coupling to fermion mass; however in this scenario none of the proposed new colliders could make these measurements.) The Higgs quantum numbers, total width, possible CP-violating effects and the Higgs self-couplings remain critical and achievable goals for the 500 GeV program.

   The need for precision measurements and for ultimate increase in the energy are similar to those in scenario 2.

4. *There is no Higgs boson and no evidence for supersymmetry at the LHC.*

   At 500 GeV, there remain loopholes to close. A possible Higgs with invisible decays can be sought. The anomalous WWV and ttZ couplings must be measured.

   A return to the Z pole and WW threshold will be needed since in this scenario, we are casting in the dark for hints on the new physics and the precision e^+e^- measurements will be crucial.

5. *We have a Higgs, evidence for supersymmetry, and other new physics signatures all superimposed.*

   The world is so complex that the precision given by the linear collider will be essential for disentangling the new physics. The LC and LHC with their complementary strengths will both play essential roles, and their programs will remain viable for years.

Some of the scenarios are better developed, and some have larger sets of measurements that we can envision. But, at least in my view, there are none for which the LC is not needed, and none for which there are not needed measurements at $/sim500$ GeV or below. Even in the case that we see little new (e.g. only a Higgs boson or nothing new at all), we still have to understand why the SM seems to work so well despite its many theoretical shortcomings.

4. Issues for the international high energy physics community

The decisions on a linear collider are likely to be made over the next few years. The TESLA proposal will be submitted to the German government in early 2001, and a recommendation may be expected roughly a year after. The JLC proposal is advancing, with work on milestones, sites and costs now underway. The R&D program for the NLC is laid out for the next three years, with a proposal thought feasible in 2003 – 2004.

All three regions are currently engaged in studies of long-term ($\sim$20 years) physics issues, and the range of facilities that might be proposed to address them. These studies should be complete in about a year. In addition to a TeV scale linear collider, these discussions address other possible projects: a muon storage ring/neutrino source, a muon collider, a multi-TeV two-beam linear e^+e^-collider, a very large hadron collider and new large underground laboratories. According to a recent HEPAP review [37], none of the alternates to the LC is expected to be ready for a technical proposal before the decade beginning in 2010. In addition, CERN has begun to evaluate its program after the LHC is operational. In addition to the projects listed above, CERN might consider substantial upgrades to the LHC energy or luminosity.

The following comments reflect some personal views on how we may approach a decision on the LC.

1. *Should the LC be the next world high energy machine?*

   I believe that it is inevitable that the LC decision will be the next to be taken by the world community. Real proposals are being made and these will be considered in the next few years. There are no proposals for alternate colliders that can be made on the time scale for LC consideration. Of course, it is not necessary that all regions propose a LC in their region, or even propose substantial financial engagement. But each region will have to make a decision on how – or whether – to address the LC issue soon. A region may opt out of the LC process, but will this will not alter the worldwide timetable for decision. This means that the physics planning and priorities activities now underway in each region have very special urgency, to bring some coherent community view of the future facilities we need..

   We should expect that no more than one LC will be built worldwide. Gaining approval for the project in any region will be enhanced by support from all regions. There could be an argument to forego engagement with a LC propos-

als in some region in favor of some other project, but there is little historical precedent to suggest that such a strategy would enhance the later, alternative project.

2. *Is the linear collider too expensive?* The fate of the SSC and the 1999 cost estimate of the NLC has led some, particularly in the US, to worry that a LC proposal will have difficulties in gaining government approval. This is of course a generic problem, since it is likely that the cost of any new high energy collider will have a multi-billion dollar cost. We will not hide from this problem by substitution of another project in place of the LC.

Some worry that the cost is the major reason for an initial first phase at 500 GeV, and that the physics needs may be insufficiently addressed in that first phase. I believe that in the past few years, we have come to a qualitatively new understanding that there is excellent physics justification for the 500 GeV machine. As indicated in the scenarios discussion, this now seems more independent of the specific discoveries at the LHC. The recent precision measurements have made it much less likely that the first indications of new physics would occur at a scale much above 500 GeV. The studies of supersymmetry seem assured to be rich at 500 GeV. Alternate models of new physics seem to have some observable consequences at 500 GeV. Making these arguments for the 500 GeV first phase to the broad HEP community is an important responsibility of the proponents of a LC.

The cost of the LC is a factor, and any ways to control the cost are worth pursuit. However, the inevitablity of future need for higher energy should dictate that we design the upgradability into the initial stage.

3. *Where will the LC be?* There have been numerous comments that there will be only one LC and that all regions should support its construction at any site. In practice, most would strongly prefer that it be built in one's own region. However, we must realize that a final decision will be taken with not only scientific considerations in play. A major factor will be which region is willing to pay the largest share of the cost.

If we are to have a viable LC program, and to retain scientific health in all regions, it will be imperative that the LC is a worldwide collaboration, both for the accelerator and for the experiments. Each region needs some frontier high energy collider activity to keep its program healthy. This can be enhanced in part through true inter-regional collaboration on each major new facility.

For the overall health of the community, it is preferrable to avoid putting most of the contemporaneous frontier facilities in one region.

4. *How can international collaboration on accelerator projects be achieved?*

Internationalism will mean that compromises are necessary; although a facility will be sited in one region, other regions should undertake major responsibilities in its design, construction, and operation. This is in part necessary to

keep a healthy community of accelerator scientists in each region. So, I imagine that for the LC, major subsystems should be taken by each region as primary responsibilities from start to finish. The global accelerator concept put forth by ICFA is a valuable start in defining the process by which this inter-regional collaboration could occur. Much work remains to make this model possible. Accelerator projects are intrinsically more tightly controlled and managed than detector projects, but the experience with international detectors is valuable. So also are the international contributions to the LHC accelerator, but what is needed to give each region a truly crucial stake in the LC is beyond what was attempted at the LHC.

5. *Technical evaluations of LC proposals*

The panel discussion of some Lab directors at this workshop highlighted a proposal for a technical review of all LC proposals before approval or site decisions are made. Such a review is imagined to start around the end of 2001. It would focus on the technical solutions for all phases of the project, risks and needed R&D. It should probably address costs in some common currency. Upgrade paths would be useful to examine. The technical review process should not address specific site issues, or attempt to make final decisions on the preferred technology beyond assessment of risk or cost factors. The technical review process should be seen as an important set of considerations that will inform the physics community and guide governmental bodies as they undertake to make a decision on approving and siting a collider. Details of how this process would work remain to be worked out. What body would charge the review panel, choose its members and receive its report? What timescale should be adopted for its activities that will not place any particular region at disadvantage technically or politically? But the benefit to the world HEP community and those who need to understand the technological and cost issues from this review are large, and we should encourage the formation of this process.

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